with black crystals attached, and grinding it with a polished sapphire, it readily scratched the same. If a larger quantity of material comes to hand, the writer will have polished a diamond with the powder of the meteorite, using a new wheel for the purpose. The writer has not seen the paper of MM. Latchinoff and Jorefeif, but there seems to be every reason to substantiate their conclusions.

These facts are of especial interest, since on Jan. 15, 1887, Prof. L. Fletcher, curator of the Mineralogical Department of the British Museum, read before the Mineralogical Society of England a paper on a meteorite which was found in the sub-district of Youndegin, Australia, in 1884, and in which he stated he had found a new form of graphite of cubic form, with the hardness of 2.5 and a specific gravity of 2.12. To this he gave the name of 'cliftonite,' calling attention, also, to the fact that Haidinger, in 1846, had found what he described as graphite pseudomorph after iron pyrites (*Poggendorf Annalen*, 1846, lxvii. p. 437), obtained by him from a nodule of graphite which had dropped out of the Arva meteorite. Gustav Rose (*Beschreibung und Entheilung der Meteoriten*, 1864, p. 40; *Poggendorf Annalen*, 1873) expressed an opinion that this mode of replacement of the cube edges on these crystals was suggestive of holo-symmetry rather than hemi-symmetry, and that this interpretation would exclude iron pyrites as a possible antecedent mineral.

The cliftonite was readily examined with a ¼-inch objective; and from its structure Professor Fletcher concluded, that, while it is different from native graphite, the sharpness, separateness, and completeness of the crystal, the brightness of the faces, the delicacy of the acicular projections, and especially of the obtuse, almost flat, square pyramids, or some of the faces, are quite sufficient to prove that the form has never had any other than its present tenants; in other words, that it is not a pseudomorph. When in cubes, the diamond has faces not very unlike those of the Youndegin crystals, and shows a similar bevelling of its edges by the rounded tetrahexedra. Again: Professor Fletcher says it might be argued, that, during a hurried crystallization of the carbon, circumstances initially favorable to the formation of the diamond had finally permitted the existence of carbon in a graphitic form only. He had also found distinct graphitic crystals, cube octahedrous in form, in the Cocke and Sevier County (Tenn.) meteorites.

When we consider that only a few meteorites have been examined for this mineral, we have reason to expect some interesting results in the future. GEORGE F. KUNZ.

New York, March 6.

A Pseudo-Meteorite.

THROUGH the kindness of Dr. DeWitt Webb of St. Augustine, Fla., I have been able to examine a portion of the so-called 'meteoric stone,' weighing over two hundred pounds, which was said to have been seen to fall in an old cultivated field near Middleburgh, Clay County, Fla., and which was exhibited at the Subtropical Exposition at Jacksonville, Fla. It is a concretionary limonite, and not of meteoric origin. GEORGE F. KUNZ.

New York, March 6.

Monocular vs. Binocular Vision.

AS a constant student of binocular phenomena, I have been much interested in Mr. Hyslop's letter in *Science* of Feb. 10. I have repeated the experiment illustrated by his Fig. 1, and confirmed his results. But I do not think they are to be explained by any supposed struggle between monocular and binocular vision, but in a far more obvious way, which, in fact, he himself suggests.

In binocular combination of such simple figures as circles, where the means of estimating distance is reduced to ocular convergence alone, the estimate is very imperfect and uncertain. Our knowledge so interferes with our visual judgment that we are apt to over-estimate the distance. In fact, many persons even find a difficulty in seeing the combined binocular image any nearer than the two monocular images. As long as attention is fixed on the combined circle, the homogeneous image of the needle will seem beyond, as it ought. This will be much more distinct if we range the point of sight back and forth, combining successively the needle-points and the circles. But when we transfer attention wholly to the double images of the needle, these latter will sometimes appear nearer than the circle; not, however, because the needle seems nearer than before, but because the circle drops to the plane of the paper, where it tends to go, anyhow.

The experiment illustrated by his second figure I cannot confirm. It is true that experiment with his figures as drawn in *Science* confirms his results, but this is only because the figures are badly drawn. The positions of the two small circles *b* and *c* are not symmetrical. When accurately drawn, I find, on combining, that the small circle and the large circle appear exactly on the same plane. My son, aged eighteen, and well practised in binocular experiments, confirms my results perfectly. Whether Mr. Hyslop's original figures were imperfect, or have been only badly copied, I know not; but the wonderful distinctness with which binocular combination will bring out and exaggerate the smallest differences in apparently similar figures, is well known.

JOSEPH LECONTE.

Berkeley, Cal., Feb. 22.

The Scientific Swindler Again.

THE following from the *Indianapolis Journal* of Feb. 24 may be of interest to those who have been the victims of the swindler so extensively advertised by your own and other journals: "The book-thief who has, under the names of W. R. Taggart, Professor Cameron, Professor Douglass, and various *aliases*, travelled over the country, representing himself as a scientific student, and borrowing valuable books, has been arrested in Cincinnati, where he gave the name of Otto Syrski. He was recognized yesterday by Professor Collett of this city, who was one of his victims. Professor Collet learned where his books had been sold, and will probably recover them." It is to be hoped that this will stop his operations, at least for a time. A. W. BUTLER.

Brookville, Ind., March 1.

A Critique of Psycho-Physic Methods.

DR. JOSEPH JASTROW, in the second number of the *Journal of Psychology*, discusses the principal psycho-physic methods now in use, and advocates a thorough reform of the science of psycho-physics. One of the principal conclusions at which he arrives is that no such thing as a differential threshold exists; that is to say, that there is no definite point at which the difference of two sensations ceases to be perceptible. Dr. Jastrow's arguments fail to convince us. He says, "The threshold is described as a point not exactly constant, but nearly so: above it all differences can be felt, below it all differences vanish into unconsciousness. No matter whether little or much below this point, they are utterly lost. It is idle to say, as Fechner at times does, that they differ in the amount of additional stimulation necessary to bring them up into consciousness, unless you mean that the series below the so-called threshold is an exact continuation of the series above it; and, if you do mean this, then the threshold loses all its distinguishing peculiarities, and ceases to exist." Further on, in discussing the theory of the right and wrong cases, he says, "It has been proved that the ratio of wrong answers increases as the difference between the stimuli decreases; but the 'threshold theory' claims that this last fails to hold after this difference has been diminished below a certain ratio."

In considering these objections, I may be allowed to treat two classes of sensations separately: first, the judgment that a difference exists is based on a sudden change in the character of the sensation either in space or time; second, the judgment refers to sensations separate in space or time or in both. As an example of the former, we may assume two adjoining fields of various colors or various intensities of light, or a sound suddenly increasing in intensity or height. The threshold theory says there is a certain difference between these adjoining sensations below which no difference will be perceived. Practically this is admitted by Jastrow. In trying to meet such an argument, he first says that there exists only an average threshold; i.e., the average smallest perceptible proportion of intensity or wave-length of the two sensations on which the observer is able to form a judgment. He continues, "Here you either (1) tacitly assume that not many observations are to be taken, or that (2) no matter how many observations were made, no mistake would ever occur."

The arguments of the advocates of the threshold theory are

somewhat different from what Jastrow would make us believe. In the first class of sensations there are two reasons for the existence of a threshold, — a physiological and a psychological. As a balance has a certain limit of accuracy beyond which it does not show differences of weight of two bodies, so our organs of sensation are not able to show differences between stimuli varying only to a very small extent. This is the physiological threshold. But, besides, the advocate of the threshold theory says it is necessary that the sensations should differ to a certain degree, else they cannot be distinguished. He does not say, however, as Jastrow assumes, that the magnitude of this least perceptible difference is the same at any moment. On the contrary, it depends on the state of mind of the person, and varies just as Jastrow's sensibility varies, every moment having its own threshold, the average of which is the average threshold of the observer.

The theory of the threshold may be summed up in the following remarks: —

Two sensations are given, the difference of which is to be judged upon. The judgment can have various characteristics. Either a certain phenomenon is observed which has no immediate connection with the sensations to be compared (for instance, the line dividing two fields of various colors is observed), or the sensations are separate in space and time. In this case the conception of the former is compared with the latter sensation. In the former case the physiological threshold is the main consideration, and for this reason it may be omitted in these brief remarks.

In the latter case let the sensations S_1 and S_2 be given, which are produced by the stimuli s_1 and s_2. Let S_1 be the first to be observed. In making the comparison, S_1 will not be correctly remembered; but the probability that another and similar sensation, S_x, which would correspond to the stimulus s_x, is produced, will be

$$W = \int_{S_x} f(s_x, s_1, C)\, ds,$$

the constant depending upon the conditions of the experiment.

Experiments show that W increases when the difference between s_1 and s_x decreases. Further experiments show that when the two stimuli s_1 and s_2 differ but slightly, in a great number of cases the observer will judge $S_1 = S_2$. According to the theory of probability, W_{S_2} is only very small as compared to all other possible reproductions. Therefore the only possible explanation of the fact that the judgment $S_1 = S_2$ is comparatively frequent, is, that not only in those instances when the conception S_2 is reproduced are both judged to be identical, but that sensations varying only slightly from S_2 cannot be distinguished from it; and the task of psycho-physic methods is to find the limits of these variations. Mathematically the number of observations in which both sensations are considered the same is expressed by the following formula: —

$$W_1 = \int_{s_2 - \delta}^{s_2 + \delta_1} f(s, s_1, C)\, ds.$$

δ_1 and δ are the upper and lower thresholds respectively. This explanation agrees exactly with the observed fact, that slightly different stimuli cannot be distinguished; and Jastrow's objections are founded on a misconception of the mathematical basis of the theory. No advocate of the threshold theory assumes, as Jastrow supposes, that below the threshold the probability of a greater error is the same as that of a smaller error.

In another passage of his critique, Jastrow rejects the use of doubtful cases in the theory of right and wrong cases. It seems to me that his objections cannot be accepted. The fact is, that in a number of cases doubtful answers must be given. In his paper he says, and rightly, that the confidence is increasing with the difference of the sensations. Now, the answer 'doubtful' is nothing else than an expression of the degree of confidence; and, according to the above formula, the proper way to include these answers in the theory is to assume a second threshold which shows the limit of doubtful cases, and this has been successfully done.

It will easily be seen that variations of a sensation such as assumed by the theory outlined above always occur, and that they must prevail in all psycho-physic experiments except in the first class.

Dr. Jastrow's suggestion to measure the sensibility by psycho-physic methods is a good one. It has been successfully applied for measuring various degrees of attention, and the writer fully agrees with Dr. Jastrow's opinion that this is the most promising field of psycho-physic research. DR. FRANZ BOAS.

New York, March 1.

American and Foreign Microscopes.

MY attention having been called to the 'Complaint' in *Science* for Dec. 2, 1887, and the following articles on microscopes, the facts did not seem to me fully presented therein. I immediately addressed the following questions to more than twenty of the leading colleges of the country, the Department of Agriculture, Geological Survey, and Microscopical Society of Washington, D.C., and Messrs. Wolle and Smith, two of the oldest microscopists in the country. The results are herewith presented, with my own ideas on the subject.

The questions were, 1. How many microscopes of American make have you? [659.] 2. How many of foreign make? [434.] 3. How many without a joint? [309.] 4. Do your students work standing, or sitting? 5. Is the instrument used in an inclined position to any extent?

The figures in brackets give the sums total of the replies. Pennsylvania University reports 100 American, 3 foreign; Michigan, 120 American, 30 foreign. Of the foreign instruments, 108 belong to Harvard, and 135 to Bryn Mawr, Johns Hopkins, and Massachusetts Institute of Technology. About 40 jointed instruments are reported used in the upright position; more than two-thirds of the whole number are used inclined. To No. 4, the answer "Sitting," is almost universal; "Standing or sitting," a few. The following extracts from the replies are pertinent: —

"I prefer to work it upright, and teach my students so, but they will incline it whenever possible."

"When long at work, I prefer a vertical tube; but I find for young students the inclined position and the rack and pinion extremely desirable."

"Only by unfortunates. Of course, the joint is a convenience, but is not, in my opinion, essential." — HARVARD COLLEGE, in answer to No. 5.

"The instruments are used almost exclusively in the upright position, the tables being low enough to permit of such use with ease." — UNIVERSITY OF NEBRASKA.

"Mostly foreign instruments, generally inclined, prefer inclined; would use it inclined if I could" [of upright instruments]. — GEOLOGICAL SURVEY.

"The latest purchases are American, which are now preferred." — ALBANY.

"Personally, I believe the best instruments are made in this country." — UNIVERSITY OF MICHIGAN.

"In my laboratory (physiology and hygiene), we use forty. I bought the first in 1876, foreign because then cheaper. In four years they were all worthless. We then bought American: they have stood more rough usage, and had fewer repairs necessary, than any others. My work is especially trying on account of the frequency with which acids must be used."

"I believe the eye is more nearly in its normal and best position when the microscope is inclined." — PRINCETON.

"My constant companion at my table is Zentmayer's army microscope. Have used it twelve or more years, always inclined, or very rarely vertical." — F. WOLLE.

"Twenty-five years ago I got Powell and Lealand's stands. I seldom use their objectives. For long years I have preferred American objectives. I have recently seen letters from purchasers of Zeiss apochromatics, confessing that Spencer's most recent glasses fully hold their own, and at less prices." — H. L. SMITH.

"The facility to incline when needed is indispensable." — J. G. HUNT.

In 1862 I saw much of Dr. Hunt, then unsurpassed as a histologist. He used a Beck best, inclined, in continuous daily work. His experience assisted in the construction of the American Centennial instrument, which he has since used. This is an instance of an elaborate tool employed in actual, original, and long-continued work. After this came the Beck International, costing seventeen